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W. J. GREENSTREET, M.A.

WITH THE CO-OPERATION OF

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AND

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MATHEMATICAL LOGIC.*

BY F. P. RAMSEY, M.A.

I HAVE been asked to speak about developments in Mathematical Logic since the publication of *Principia Mathematica*, and I think it would be most interesting if, instead of describing various definite improvements of detail, I were to discuss in outline the work which has been done on entirely different lines, and claims to supersede altogether the position taken up by Whitehead and Russell, as to the nature of mathematics and its logical foundations.

Let me begin by recalling what Whitehead and Russell's view is; it is that mathematics is part of formal logic, that all the ideas of pure mathematics can be defined in terms which are not distinctively mathematical, but involved in complicated thought of any description, and that all the propositions of mathematics can be deduced from propositions of formal logic, such as that if p is true, then either p or q is true. This view seems to me in itself plausible, for so soon as logic has been developed beyond its old syllogistic nucleus, we shall expect to have besides the forms "all men are mortal," "some men are mortal," the numerical forms, "two men are mortal" and "three men are mortal," and number will have to be included in formal logic.

Frege was the first to maintain that mathematics was part of logic, and to construct a detailed theory on that basis. But he fell foul of the famous contradictions of the theory of aggregates, and it appeared that contradictory consequences could be deduced from his primitive propositions. Whitehead and Russell escaped this fate by introducing their Theory of Types, of which it is impossible here to give an adequate account. But one of its implications must be explained if later developments are to be intelligible.

Suppose we have a set of characteristics, given as all characteristics of a certain sort, say A, then we can ask about anything, whether it has a characteristic of the sort A. If it has, this will be another characteristic of it, and the question arises whether this characteristic, the characteristic of having a characteristic of the kind A, can itself be of the kind A, seeing that it presupposes the totality of such characteristics. The Theory of Types held that it could not, and that we could only escape contradiction by saying that it was a characteristic of higher order, and could not be included in any statement

* A Paper read before the British Association, Oxford, 1926

about all characteristics of lower order. And more generally that any statement about all characteristics must be regarded as meaning all of a certain order. This seemed in itself plausible, and also the only way of avoiding certain contradictions which arose from confusing these orders of characteristics. Whitehead and Russell also hold that statements about classes or aggregates are to be regarded as really about the characteristics which define the classes (a class being always given as the class of things possessing a certain character), so that any statement about all classes will be really about all characteristics, and will be liable to the same difficulties with regard to the order of these characteristics.

Such a theory enables us easily to avoid the contradictions of the Theory of Aggregates, but it has also the unfortunate consequence of invalidating an ordinary and important type of mathematical argument, the sort of argument by which we ultimately establish the existence of the upper bound of an aggregate, or the existence of the limit of a bounded monotonic sequence. It is usual to deduce these propositions from the principle of Dedekindian section, that if the real numbers are divided completely into an upper and a lower class, there must be a dividing number which is either the least of the upper class or the greatest of the lower. This in turn is proved by regarding real numbers as sections of rationals; sections of rationals are a particular kind of classes of rationals, and hence a statement about real numbers will be a statement about a kind of classes of rationals, that is about a kind of characteristics of rationals, and the characteristics in question will have to be limited to be of a certain order.

Now suppose we have an aggregate E of real numbers; that will be a class of characteristics of rationals. ξ, the upper bound of E, is defined as a section of rationals which is the sum of the members of E; *i.e.* ξ is a section whose members are all those rationals which are members of any member of E, that is all those rationals which have the characteristic of having any of the characteristics which give the members of E. So the upper bound ξ is a section whose defining characteristic is one of higher order than those of the members of E. Hence if all real numbers means all sections of rationals defined by characteristics of a certain order, the upper bound will, in general, be a section of rationals defined by a characteristic of higher order, and will not be a real number. This means that analysis as ordinarily understood is entirely grounded on a fallacious kind of argument, which when applied in other fields leads to self-contradictory results.

This unfortunate consequence of the Theory of Types Whitehead and Russell tried to avoid by introducing the Axiom of Reducibility, which asserted that to any characteristic of higher order there was an equivalent characteristic of the lowest order; equivalent in the sense that everything that has the one has the other, so that they define the same class. The upper bound, which we saw was a class of rationals defined by a characteristic of higher order, would then also be defined by the equivalent characteristic of lower order, and would be a real number. Unfortunately the axiom is certainly not self-evident, and there is no reason whatever to suppose it true. If it were true this would only be, so to speak, a happy accident, and it would not be a logical truth like the other primitive propositions.

In the second edition of *Principia Mathematica*, of which the first volume was published last year, Mr. Russell has shown how mathematical induction, for which the Axiom of Reducibility seemed also to be required, can be established without it, but he does not hold out any hope of similar success with the Theory of Real Numbers, for which the ingenious method used for the whole numbers is not available. The matter is thus left in a profoundly unsatisfactory condition.

This was pointed out by Weyl, who published in 1918 a little book called *Das Kontinuum*, in which he rejected the Axiom of Reducibility and accepted

the consequence that ordinary analysis was wrong. He showed, however, that various theorems, such as Cauchy's General Principle of Convergence, could still be proved.

Since then Weyl has changed his view and become a follower of Brouwer, the leader of what is called the Intuitionist school, whose chief doctrine is the denial of the Law of Excluded Middle, that every proposition is either true or false.* This is denied apparently because it is thought impossible to know such a thing *a priori*, and equally impossible to know it by experience, because if we do not know either that it is true or that it is false, we cannot verify that it is either true or false. Brouwer would refuse to agree that either it was raining or it was not raining, unless he had looked to see. Although it is certainly difficult to give a philosophical explanation of our knowledge of the laws of logic, I cannot persuade myself that I do not know for certain that the law of excluded middle is true; of course, it cannot be proved, although Aristotle gave the following ingenious argument in its favour. If a proposition is neither true nor false, let us call it doubtful; but then if the law of excluded middle be false, it need not be either doubtful or not doubtful, so we shall have not merely three possibilities but four, that it is true, that it is false, that it is doubtful, and that it is neither true, false, nor doubtful. And so on *ad infinitum*.

But if it be answered why not ?, there is clearly nothing more to be said, and I do not see how any common basis can be found from which to discuss the matter. The cases in which Brouwer thinks the Law of Excluded Middle false, are ones in which, as I should say, we could not tell whether the proposition was true or false; for instance, is $2^{\sqrt{2}}$ rational or irrational ?; we cannot tell, but Brouwer would say it was neither. We cannot find integers m, n, so that $\frac{m}{n}=2^{\sqrt{2}}$; therefore it is not rational: and we cannot show that it is impossible to find such integers, therefore it is not irrational. I cannot see that the matter is not settled by saying that it is either rational or irrational, but we can't tell which. The denial of the Law of Excluded Middle renders illegitimate the argument called a dilemma, in which something is shown to follow from one hypothesis, and also from the contradictory of that hypothesis, and it is concluded that it is true unconditionally. Thus Brouwer is unable to justify much of ordinary mathematics, and his conclusions are even more sceptical than those of Weyl's first theory.

Weyl's second theory is very like Brouwer's, but he seems to deny the Law of Excluded Middle for different reasons, and in a less general way. He does not appear to deny that any proposition is either true or false, but denies the derived law that either every number has a given property, or at least one number does not have it. He explains his denial first of all for real numbers in the following way. A real number is given by a sequence of integers, for instance as an infinite decimal; this sequence we can conceive as generated either by a law, or by successive acts of choice. If now we say there is a real number or sequence having a certain property, this can only mean that we have found a law giving one; but if we say all sequences have a property, we mean that to have the property is part of the essence of a sequence, and therefore belongs to sequences arising not only by laws, but from free acts of choice. Hence it is not true that either all sequences have the property, or there is a sequence not having it. For the meaning of sequence is different in the two clauses. But I do not see why it should not be possible to use the word consistently. However this may be, nothing similar can be urged about the whole numbers which are not defined by sequences, and so another more

* For instance, as the White Knight said, "Everybody that hears me sing—either it brings the *tears* into their eyes or else——" "Or else what?" said Alice, for the Knight had made a sudden pause. "Or else it doesn't, you know."

fundamental reason is put forward for denying the Law of Excluded Middle. This is that general and existential propositions are not really propositions at all. If I say "2 is a prime number," that is a genuine judgment asserting a fact; but if I say "there is a prime number" or "all numbers are prime," I am not expressing a judgment at all. If, Weyl says, knowledge is a treasure, the existential proposition is a paper attesting the existence of a treasure, but not saying where it is. We can only say, "there is a prime number," when we have previously said, "this is a prime number," and forgotten or chosen to disregard which particular number it was. Hence it is never legitimate to say, "there is a so-and-so," unless we are in possession of a construction for actually finding one. In consequence, mathematics has to be very considerably altered; for instance, it is impossible to have a function of a real variable with more than a finite number of discontinuities. The foundation on which this rests, namely the view that existential and general propositions are not genuine judgments, I shall come back to later.

But first I must say something of the system of Hilbert and his followers, which is designed to put an end to such scepticism once and for all. This is to be done by regarding higher mathematics as the manipulation of meaningless symbols according to fixed rules. We start with certain rows of symbols, called axioms: from these we can derive others by substituting certain symbols called constants for others called variables, and by proceeding from the pair of formulae p, if p then q, to the formula q.

Mathematics proper is thus regarded as a sort of game, played with meaningless marks on paper rather like noughts and crosses, but besides this there will be another subject called metamathematics, which is not meaningless, but consists of real assertions about mathematics, telling us that this or that formula can or cannot be obtained from the axioms according to the rules of deduction. The most important theorem of metamathematics is that it is not possible to deduce a contradiction from the axioms, where by a contradiction is meant a formula with a certain kind of shape, which can be taken to be $0 \neq 0$. This I understand Hilbert has proved, and has so removed the possibility of contradictions and scepticism based on them.

Now, whatever else a mathematician is doing, he is certainly making marks on paper, and so this point of view consists of nothing but the truth; but it is hard to suppose it the whole truth. There must be some reason for the choice of axioms, and some reason why the particular mark $0 \neq 0$ is regarded with such abhorrence. This last point can, however, be explained by the fact that the axioms would allow anything whatever to be deduced from $0 \neq 0$, so that if $0 \neq 0$ could be proved, anything whatever could be proved, which would end the game for ever, which would be very boring for posterity. Again, it may be asked whether it is really possible to prove that the axioms do not lead to contradiction, since nothing can be proved unless some principles are taken for granted and assumed not to lead to contradiction. This objection is admitted, but it is contended that the principles used in the metamathematical proof that the axioms of mathematics do not lead to contradiction, are so obviously true that not even the sceptics can doubt them. For they all relate not to abstract or infinitely complex things, but to marks on paper, and though anyone may doubt whether a subclass of a certain sort of infinite series must have a first term, no one can doubt that if $=$ occurs on a page, there is a place on the page where it occurs for the first time.

But, granting all this, it must still be asked what use or merit there is in this game the mathematician plays, if it is really a game and not a form of knowledge; and the only answer which is given is that some of the mathematician's formulae have or can be given meaning, and that if these can be proved in the symbolic system their meanings will be true. For Hilbert shares Weyl's opinion that general and existential propositions are meaningless, so that the only parts of mathematics which mean anything are particular assertions about

finite integers, such as "47 is a prime" and conjunctions and disjunctions of a finite number of such assertions like "there is a prime between 50 and 100," which can be regarded as meaning, "either 51 is a prime or 52 is a prime, etc., up to, or 99 is a prime." But as all such propositions of simple arithmetic can be easily proved without using higher mathematics at all, this use for it cannot be of great importance. And it seems that although Hilbert's work provides a new and powerful method, which he has successfully applied to the Continuum problem, as a philosophy of mathematics it can hardly be regarded as adequate.

We see then that these authorities, great as are the differences between them, are agreed that mathematical analysis as ordinarily taught cannot be regarded as a body of truth, but is either false or at best a meaningless game with marks on paper; and this means, I think, that mathematicians in this country should give some attention to their opinions, and try to find some way of meeting the situation.

Let us then consider what sort of a defence can be made for classical mathematics, and Russell's philosophy of it.

We must begin with what appears to be the crucial question, the meaning of general and existential propositions, about which Hilbert and Weyl take substantially the same view. Weyl says that an existential proposition is not a judgment, but an abstract of a judgment, and that a general proposition is a sort of cheque which can be cashed for a real judgment when an instance of it occurs.

Hilbert, less metaphorically, says that they are ideal propositions, and fulfil the same function in logic as ideal elements in various branches of mathematics. He explains their origin in this sort of way; a genuine finite proposition such as "There is a prime between 50 and 100," we write, "There is a prime which is greater than 50 and less than 100," which appears to contain a part, "There is a prime which is greater than 50." This, however, would mean, "51 is a prime, or 52 is a prime, etc., *ad inf.*," and so be an infinite logical sum, which, like an infinite algebraic sum, is first of all meaningless, and can only be given a secondary meaning subject to certain conditions of convergence. But the introduction of these meaningless forms so simplifies the rules of inference, that it is convenient to retain them, regarding them as ideals, for which a consistency theorem must be proved.

In this view of the matter there seem to me to be several difficulties. First it is hard to see what use these ideals can be supposed to be; for mathematics proper appears to be reduced to elementary arithmetic, not even algebra being admitted, for the essence of algebra is to make general assertions. Now any statement of elementary arithmetic can be easily tested or proved without using higher mathematics, which if it be supposed to exist solely for the sake of simple arithmetic, seems entirely pointless. Secondly, it is hard to see how the notion of an ideal can fail to presuppose the possibility of general knowledge. For the justification of ideals lies in the fact that *all* propositions not containing ideals which can be proved by means of them are true. And so Hilbert's metamathematics, which is agreed to be genuine truth, is bound to consist of general propositions about all possible mathematical proofs, which, though each proof is a finite construct, may well be infinite in number. And if, as Weyl says, an existential proposition is a paper attesting the existence of a treasure of knowledge but not saying where it is, I cannot see how we explain the utility of such a paper, except by presupposing its recipient capable of the existential knowledge that there is a treasure somewhere.

Moreover, even if Hilbert's account could be accepted so long as we confine our attention to mathematics, I do not see how it could be made plausible with regard to knowledge in general. Thus, if I tell you, "I keep a dog," you appear to obtain knowledge of a fact; trivial, but still knowledge. But "I keep a dog" must be put into logical symbolism, as "There is something which is a

dog and kept by me" ; so that the knowledge is knowledge of an existential proposition, covering the possibly infinite range of "things." Now it might possibly be maintained that my knowledge that I keep a dog arose in the sort of way Hilbert describes by my splitting off incorrectly what appears to be part of a finite proposition, such as "Rolf is a dog, and kept by me," but your knowledge cannot possibly be explained in this way, because the existential proposition expresses all you ever have known, and probably all you ever will know about the matter.

Lastly, even the apparently individual facts of simple arithmetic seem to me to be really general. For what are these numbers, that they are about? According to Hilbert marks on paper constructed out of the marks 1 and +. But this account seems to me inadequate, because if I said, "I have two dogs," that would also tell you something, you would understand the word "two," and the whole sentence could be rendered something like "There are x and y, which are my dogs and are not identical with one another." This statement appears to involve the idea of existence, and not to be about marks on paper; so that I do not see that it can be seriously held that a cardinal number which answers the question how many? is merely a mark on paper. If then we take one of these individual arithmetical facts, such as $2+2=4$, this seems to me to mean, "If the p's are two in number, and the q's also, and nothing is both a p and a q, then the number of things which are either p's or q's is four." For this is the meaning in which we must take $2+2=4$ in order to use it, as we do, to infer from I have 2 dogs and 2 cats to I have 4 pets. This apparently individual fact, $2+2=4$, then contains several elements of generality and existentiality, firstly because the p's and q's are absolutely general characteristics, and secondly because the parts of the proposition, such as "if the p's are two in number," involve as we have seen the idea of existence.

It is possible that the whole assertion that general and existential propositions cannot express genuine judgments or knowledge, is purely verbal; that it is merely being decided to emphasise the difference between individual and general propositions by refusing to use the words judgment and knowledge in connection with the latter. This, however, would be a pity, for all our natural associations to the words judgment and knowledge fit general and existential propositions as well as they do individual ones; for in either case we can feel greater or lesser degrees of conviction about the matter, and in either case we can be in some sense right or wrong. And the suggestion which is implied, that general and existential knowledge exists simply for the sake of individual knowledge, seems to me entirely false. In theorising what we principally admire is generality, and in ordinary life it may be quite sufficient to know the existential proposition that there is a bull somewhere in a certain field, and there may be no further advantage in knowing that it is this bull and here in the field, instead of merely a bull somewhere.

How then are we to explain general and existential propositions? I do not think we can do better than accept the view which has been put forward by Wittgenstein as a consequence of his theory of propositions in general. He explains them by reference to what may be called atomic propositions, which assert the simplest possible sort of fact, and could be expressed without using even implicitly any logical terms such as or, if, all, some. "This is red" is perhaps an instance of an atomic proposition. Suppose now we have, say, n atomic propositions; with regard to their truth or falsity, there are 2^n mutually exclusive ultimate possibilities. Let us call these the truth-possibilities of the n atomic propositions; then we can take any sub-set of these truth-possibilities and assert that it is a possibility out of this sub-set which is, in fact, realised. We can choose this sub-set of possibilities in which we assert the truth to lie in 2^{2^n} ways; and these will be all the propositions we can build up out of these n atomic propositions. Thus to take a simple instance, "if p, then q" expresses agreement with the three possibilities, that both p and q are

true, that p is false and q true, and that p is false and q false, and denies the remaining possibility that p is true and q false.

We can easily see that from this point of view there is a redundancy in all ordinary logical notations, because we can write in many different ways what is essentially the same proposition, expressing agreement and disagreement with the same sets of possibilities.

Mr. Wittgenstein holds that all propositions express agreement and disagreement with truth-possibilities of atomic propositions, or, as we say, are truth-functions of atomic propositions; although often the atomic propositions in question are not enumerated, but determined as all values of a certain propositional function. Thus the propositional function "x is red" determines a collection of propositions which are its values, and we can assert that all or at least one of these values are true by saying, "For all x, x is red" and "There is an x such that x is red" respectively. That is to say, if we could enumerate the values of x as $a, b \ldots z$, "For all x, x is red" would be equivalent to the proposition "a is red and b is red and . . . and z is red." It is clear, of course, that the state of mind of a man using the one expression differs in several respects from that of a man using the other, but what might be called the logical meaning of the statement, the fact which is asserted to be, is the same in the two cases.

It is impossible to discuss now all the arguments which might be used against this view, but something must be said about the argument of Hilbert, that if the variable has an infinite number of values, if, that is to say, there are an infinite number of things in the world of the logical type in question, we have here an infinite logical sum or product which, like an infinite algebraic sum or product, is initially meaningless and can only be given a meaning in an indirect way. This seems to me to rest on a false analogy; the logical sum of a set of propositions is the proposition that one of the set at least is true, and it doesn't appear to matter whether the set is finite or infinite. It is not like an algebraic sum to which finitude is essential, since it is extended step by step from the sum of two terms. To say that anything possibly involving an infinity of any kind must be meaningless is to declare in advance that any real theory of aggregates is impossible.

Apart from providing a simple account of existential and general propositions, Wittgenstein's theory settles another question of the first importance by explaining precisely the peculiar nature of logical propositions. When Mr. Russell first said that mathematics could be reduced to logic, his view of logic was that it consisted of all true absolutely general propositions, propositions that is, which contained no material (as opposed to logical) constants. Later he abandoned this view, because it was clear that some further characteristic besides generality was required. For it would be possible to describe the whole world without mentioning any particular thing, and clearly something may by chance be true of anything whatever without having the character of necessity which belongs to the truths of logic.

If, then, we are to understand what logic, and so on Mr. Russell's view, mathematics is, we must try to define this further characteristic which may be vaguely called necessity, or from another point of view tautology. For instance, "p is either true or false" may be regarded either as necessary truth or as a mere tautology. This problem is incidentally solved by Wittgenstein's theory of propositions. Propositions, we said, expressed agreement and disagreement with the truth-possibilities of atomic propositions. Given n atomic propositions, there are 2^n truth-possibilities, and we can agree with any set of these and disagree with the remainder. There will then be two extreme cases, one in which we agree with all the possibilities, and disagree with none, the other in which we agree with none and disagree with all. The former is called a tautology, the latter a contradiction.

The simplest tautology is "p or not p"; such a statement adds nothing to

our knowledge, and does not really assert a fact at all; it is, as it were, not a real proposition, but a degenerate case. And it will be found that all propositions of logic are in this sense tautologies; and this is their distinguishing characteristic. All the primitive propositions in *Principia Mathematica* are tautologies except the Axiom of Reducibility, and the rules of deduction are such that from tautologies only tautologies can be deduced, so that were it not for the one blemish, the whole structure would consist of tautologies. We thus are brought back to the old difficulty, but it is possible to hope that this too can be removed by some modification of the Theory of Types which may result from Wittgenstein's analysis.

A theory of types must enable us to avoid the contradictions; Whitehead and Russell's theory consisted of two distinct parts, united only by being both deduced from the rather vague "Vicious-Circle Principle." The first part distinguished propositional functions according to their arguments, *i.e.* classes according to their members, the second part created the need for the Axiom of Reducibility by requiring further distinctions between orders of functions with the same type of arguments.

We can easily divide the contradictions according to which part of the theory is required for their solution, and when we have done this we find that these two sets of contradictions are distinguished in another way also. The ones solved by the first part of the theory are all purely logical; they involve no ideas but those of class, relation and number, could be stated in logical symbolism, and occur in the actual development of mathematics, when it is pursued in the right direction. Such are the contradiction of the greatest ordinal, and that of the class of classes which are not members of themselves. With regard to these Mr. Russell's solution seems inevitable.

On the other hand, the second set of contraditions are none of them purely logical or mathematical, but all involve some psychological term, such as meaning, defining, naming or asserting. They occur not in mathematics, but in thinking about mathematics; so that it is possible that they arise not from faulty logic or mathematics, but from ambiguity in the psychological or epistemological notions of meaning and asserting. Indeed, it seems that this must be the case, because examination soon convinces one that the psychological term is in every case essential to the contradiction, which could not be constructed without introducing the relation of words to their meaning or some equivalent.

If now we try to apply to the question Wittgenstein's theory of generality, we can, I think, fairly easily construct a solution along these lines. To explain this adequately would require a paper to itself, but it may be possible to give some idea of it in a few words. On Wittgenstein's theory a general proposition is equivalent to a conjunction of its instances, so that the kind of fact asserted by a general proposition is not essentially different from that asserted by a conjunction of atomic propositions. But the symbol for a general proposition means its meaning in a different way from that in which the symbol for an elementary proposition means it, because the latter contains names for all the things it is about, whereas the general propositions symbol contains only a variable standing for all its values at once. So that though the two kinds of symbol could mean the same thing, the senses of meaning in which they mean it must be different. Hence the orders of propositions will be characteristics not of what is meant, which is alone relevant in mathematics, but of the symbols used to mean it.

First-order proposition will be rather like spoken word; the same word can be both spoken and written, and the same proposition can theoretically be expressed in different orders. Applying this *mutatis mutandis* to propositional functions, we find that the typical distinctions between functions with the same arguments, apply not to what is meant, but to the relation of meaning between symbol and object signified. Hence they can be neglected in mathe-

matics, and the solution of the contradictions can be preserved in a slightly modified form, because the contradictions here relevant all have to do with the relation of meaning.

In this way I think it is possible to escape the difficulty of the Axiom of Reducibility, and remove various other more philosophical objections, which have been made by Wittgenstein, thus rehabilitating the general account of the Foundations of Mathematics given by Whitehead and Russell. But there still remains an important point in which the resulting theory must be regarded as unsatisfactory, and that is in connection with the Axiom of Infinity.

According to the authors of *Principia Mathematica* there is no way of proving that there are an infinite number of things in any logical type ; and if there are not an infinite number in any type, the whole theory of infinite aggregates, sequences, differential calculus and analysis in general breaks down. According to their theory of number, if there were only ten individuals, in the sense of number appropriate to individuals all numbers greater than ten would be identical with the null-class and so with one another. Of course there would be 2^{10} classes of individuals, and so the next type of numbers would be all right up to 2^{10}, and so by taking a high enough type any finite number can be reached.

But it will be impossible in this way to reach Aleph 0. There are various natural suggestions for getting out of this difficulty, but they all seem to lead to reconstituting the contradiction of the greatest ordinal.

It would appear then impossible to put forward analysis except as a consequence of the Axiom of Infinity ; nor do I see that this would in general be objectionable, because there would be little point in proving propositions about infinite series, unless such things existed. And on the other hand the mathematics of a world with a given finite number of members is of little theoretical interest, as all its problems can be solved by a mechanical procedure.

But a difficulty seems to me to arise in connection with elementary propositions in the theory of numbers, which can only be proved by transcendental methods, such as Dirichlet's evaluation of the class number of quadratic forms. Let us consider such a result of the form " every number has the property p," proved by transcendental methods only for the case of an infinite world ; besides this, if we knew the world only contained say, 1,000,000 things, we could prove it by testing the numbers up to 1,000,000. But suppose the world is finite and yet we do not know any upper limit to its size, then we are without any method of proving it at all.

It might be thought that we could escape this conclusion by saying that although no infinite aggregate may exist, the notion of an infinite aggregate is not self-contradictory, and therefore permissible in mathematics. But I think this suggestion is no use, for three reasons: firstly, it appears as a result of some rather difficult, but I think conclusive reasoning by Wittgenstein, that if we accept his theory of general and existential propositions (and it was only so that we could get rid of the Axiom of Reducibility), it will follow that if no infinite aggregate existed, the notion of such an aggregate would be self-contradictory ; secondly, however that may be, it is generally accepted that the only way of demonstrating that postulates are compatible, is by an existence theorem showing that there actually is and not merely might be a system of the kind postulated ; thirdly, even if it were granted that the notion of an infinite aggregate were not self-contradictory, we should have to make large alterations in our system of logic in order to validate proofs depending on constructions in terms of things which might exist but don't. The system of *Principia* would be quite inadequate.

What then can be done ? We can try to alter the proofs of such propositions, and it might therefore be interesting to try to develop a new mathematics without the axiom of infinity ; the methods to be adopted might resemble those of Brouwer and Weyl. These authorities, however, seem to me to be

sceptical about the wrong things in rejecting not the Axiom of Infinity, but the clearly tautologous Law of Excluded Middle. But I do not feel at all confident that anything could be achieved on these lines which would replace the transcendental arguments at present employed.

Another possibility is that Hilbert's general method should be adopted, and that we should use his proof, that no contradiction can be deduced from the axioms of mathematics including an equivalent of the Axiom of Infinity. We can then argue thus: whether a given number has or has not the property p can always be found out by calculation. This will give us a formal proof of the result for this particular number, which cannot contradict the general result proved from the axiom of infinity, which must therefore be valid.

But this argument will still be incomplete, for it will only apply to numbers which can be symbolised in our system. And if we are denying the axiom of infinity, there will be an upper limit to the number of marks which can be made on paper, since space and time will be finite, both in extension and divisibility, so that some numbers will be too large to be written down, and to them the proof will not apply. And these numbers being finite will be existent in a sufficiently high type, and Hilbert's theory will not help us to prove that they have the property p.

Another serious difficulty about the axiom of infinity is, that if it is false, it is difficult to see how mathematical analysis can be used in physics, which seems to require its mathematics to be true and not merely to follow from a possibly false hypothesis. But to discuss this adequately would take us too far.

As to how to carry the matter further, I have no suggestion to make; all I hope is to have made it clear that the subject is very difficult, and that the leading authorities are very sceptical as to whether pure mathematics as ordinarily taught can be logically justified, for Brouwer and Weyl say that it cannot, and Hilbert proposes only to justify it as a game with meaningless marks on paper. On the other hand, although my attempted reconstruction of the view of Whitehead and Russell overcomes, I think, many of the difficulties, it is impossible to regard it as altogether satisfactory.

King's College, Cambridge. F. P. Ramsey.

GLEANINGS FAR AND NEAR.

385. Robert Boyle was obliged systematically to overcome the evil effects of immoderate study: "the most effectual way he found to be the extraction of the square and cube roots, and especially those more laborious operations of algebra which so entirely exact the whole man, that the smallest distraction or heedlessness constrains us to renew our trouble, and re-begin the operation." [Quoted by De Morgan from Boyle's *Life* (written by himself in the third person, prefixed to Birch's five-volume edition of Boyle's *Works*, 1744.] It will be useful to remember as to contemporary chronology, that Boyle was born in the year in which Bacon died, and Newton in that in which Galileo died; Boyle being fifteen years older than Newton.—De Morgan, *Penny Cyclopaedia.*

386. Paradox. In 1641 Boyle travelled in Italy, where he employed himself "in the new paradoxes of the great star-gazer Galileo, whose ingenious books, perhaps because they could not be so otherwise, were confuted by a decree from Rome; his highness the pope, it seems, presuming, and that justly, that the infallibility of his chair extended equally to determine points in philosophy as in religion, and loathe to have the stability of that earth questioned in which he had established his kingdom. [Quoted by De Morgan from Boyle's *Life*]—De Morgan, *Penny Cyclopaedia.*

THE STIELTJES INTEGRAL IN HARMONIC ANALYSIS.*

By J. C. Burkill, M.A.

The object of this paper is to exhibit the application of the Stieltjes integral † to harmonic analysis.

For simplicity it is assumed throughout that the functions $f(x)$ which are analysed into their harmonic constituents are even functions, *i.e.* that $f(x)=f(-x)$ for all values of x. The effect of this assumption is that the trigonometrical expression of $f(x)$ will contain only *cosines* of multiples of x—sines will not appear. To any formula stated here in terms of cosines there corresponds in the general case a formula containing in addition a similar expression in sines. It is further assumed that $f(x)$ has only *regular* discontinuities, *i.e.* that for all values of x

$$f(x)=\tfrac{1}{2}\{f(x+0)+f(x-0)\}.$$

There are two classical methods of harmonic analysis. In the first place a function $f(x)$, which is periodic with period 2π and satisfies certain other conditions, may be expressed as a trigonometrical series:

$$\sum_0^\infty a_n \cos nx. \qquad\qquad (1)$$

Secondly, if it is assumed that $f(x)$, instead of being periodic, behaves in a suitable way as $x\to\infty$, then $f(x)$ may be expressed as an integral:

$$\int_0^\infty \cos xt\,\phi(t)\,dt. \qquad\qquad (2)$$

There is a more recent development which may be referred to here. In a series of papers published during the last three years Bohr has investigated a class of functions having properties of the nature of periodicity, which he calls *almost periodic*. An almost periodic function may be expressed as a series:

$$\Sigma a_n \cos \lambda_n x, \qquad\qquad (3)$$

where λ_n may be any sequence of real numbers. The expression (1) is the particular case of (3), in which $\lambda_n=n$, *i.e.* in which all the constituent frequencies are integral multiples of the smallest fundamental frequency.

The point which concerns us in this paper is that each of the expressions (1), (2), (3) is a particular case of the Stieltjes integral

$$\int_0^\infty \cos xt\,d\Phi(t). \qquad\qquad (4)$$

In (1), $\Phi(t)$ is a *step-function*, having a jump of amount a_n at $t=n$, and remaining constant between $t=n$ and $t=n+1$. ($n=0, 1, 2, \ldots$) Similarly in (3), $\Phi(t)$ is a step-function having jumps at the values λ_n of t. In (2),

$$\Phi(t)=\int_0^t \phi(t)\,dt,$$

and so is continuous.

Hence the introduction of the Stieltjes integral into harmonic analysis effects a certain unification in the subject, in that the different expressions (1), (2), (3) appear as particular cases of a single formula (4).

The conditions under which a function $f(x)$ can be expressed in the form (4) have been investigated by Hahn and Wiener. Wiener's arguments have been simplified and his results extended by Bochner and Hardy. These writers ‡

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* A paper read before the British Association, Oxford, 1926.

† E. C. Francis, *Mathematical Gazette*, March 1926.

‡ Hahn's memoir will be published in *Acta Mathematica*; Wiener, *Mathematische Zeitschrift*, 24 (1925), 575-616; Bochner and Hardy, London Mathematical Society.

between them prove very general theorems. All that can be attempted in this paper is the harmonic analysis of a class of functions for which the discussion is particularly simple. What we actually prove is:

If (*a*) $f(u)$ is integrable in every finite interval,

(*b*) $\int^{\infty}\left|\frac{f(u)}{u}\right| du$ converges,

(*c*) $f(u)$ satisfies at x one of the sufficient conditions for the convergence of Fourier series (*e.g.* $f(u)$ is of bounded variation in some neighbourhood of x),

then
$$f(x)=\int_0^{\infty}\cos xt\, d\Phi(t), \qquad (5)$$

where
$$\Phi(t)=\frac{1}{\pi}\int_{-\infty}^{\infty}\frac{\sin tu}{u}f(u)\,du. \qquad (6)$$

This is proved by combining the formula for integration by parts of a Stieltjes integral with the result known as Fourier's single integral.*

We have, integrating by parts,

$$\int_0^T \cos xt\, d\Phi(t)=\cos xT\Phi(T)+x\int_0^T \sin xt\Phi(t)\,dt. \qquad (7)$$

Substitute for Φ from (6). The first term on the right-hand side of (7) is

$$\frac{1}{2\pi}\int_{-\infty}^{\infty}\frac{f(u)}{u}\{\sin T(u+x)+\sin T(u-x)\}\,du. \qquad (8)$$

In the second term of the right-hand side of (7) the integral (6) for $\Phi(t)$ is uniformly convergent from (*b*), and so we may invert the order of integration and obtain

$$\frac{1}{\pi}\int_{-\infty}^{\infty} f(u)\left\{\frac{x}{u}\int_0^T \sin xt \sin tu\, dt\right\} du$$
$$=\frac{1}{2\pi}\int_{-\infty}^{\infty} f(u)\,\frac{x}{u}\left\{\frac{\sin T(u-x)}{u-x}-\frac{\sin T(u+x)}{u+x}\right\} du. \qquad (9)$$

Adding (8) and (9), we have

$$\int_0^T \cos xt\, d\Phi(t)=\frac{1}{2\pi}\int_{-\infty}^{\infty} f(u)\left\{\frac{\sin T(u-x)}{u-x}+\frac{\sin T(u+x)}{u+x}\right\} du.$$

Now let $T\to\infty$. From Fourier's single integral, since f is even, the limit of the right-hand side is

$$\tfrac{1}{2}\{f(x)+f(-x)\}=f(x),$$

and the theorem is proved.

It is interesting to compare this result with the classical Fourier double integral theorem.

The latter states that if $f(u)$ satisfies conditions (*a*), (*c*) and (*b'*), $\int^{\infty}|f(u)|\,du$ converges; then

$$f(x)=\int_0^{\infty}\cos xt\phi(t)\,dt.$$

The condition (*b*) being wider than (*b'*), it is seen that the Stieltjes integral formula (4) applies to a wider class of functions than the ordinary Fourier formula.

J. C. Burkill.

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* Hobson *Theory of Functions* (2nd ed.), vol. ii. 720.

NOTE ON THE TEACHING OF ANALYTICAL GEOMETRIES.

By A. Robson, M.A.

In his very interesting article on Asymptotes in the May *Gazette*, Prof. Nunn points out that the mathematician often asserts that a line always meets a conic in two points which may be at infinity, and that teachers and text-books dwell too little upon the fact that the word " point " is used in widely different senses.

He suggests a very reasonable theory of asymptotes which " seeks to keep on the right side both of the critical modern mathematician and of the schoolboy."

The object of this note is not at all to quarrel with Nunn's treatment, but merely to point out that another procedure can be adopted, even with schoolboys, which permits more freely of those generalisations, unqualified by exceptions, which are obviously desirable.

It was suggested by Prof. Hardy in his Presidential Address in 1925, that students should be " given the opportunity, while still at school, of learning more about the subject-matter out of which modern geometrical systems are built."

Although the geometries that I am going to mention are perhaps not the ones of which Prof. Hardy was thinking, I believe that their introduction is quite easy, and that it impresses upon the student the fact that " geometry " is a word which possesses a plural.

By " geometry 1 " I mean the geometry that occurs in the early chapters of current text-books on analytical geometry, in which, for example, the formula $(x_1-x_2)^2+(y_1-y_2)^2$ for the square of the distance is deduced from the theorem of Pythagoras.

" Geometry 2 " is what Prof. Hardy calls " common Cartesian geometry." In this geometry a point is a pair of real numbers (x, y), and a straight line is a class of points such that $ax+by+c=0$.

It is not difficult to show a junior set of scholarship boys that " geometry 2 " can be developed, so that its theorems are nearly the same as those of " geometry 1." It differs from " geometry 1 " chiefly in that certain results which are regarded as matters for proof in " 1 " are definitions or axioms in " 2 " ; for example, the formula for the distance becomes a definition.

It is unnecessary to proceed far with the development of the details of " geometry 2," and as soon as the student begins to protest against the futility of building up a new geometry almost identical with the first one, he can be introduced to " geometry 3." This geometry is like " 2," but has pairs of complex numbers for its points instead of real numbers. Evidently geometry 3 must be built up on the lines of geometry 2, not of geometry 1. Geometry 2 serves for the student as a stepping-stone from " 1 " to " 3."

" Geometry 4 " has triplets of real numbers (x, y, z) for its points, and " geometry 5 " has triplets of complex numbers. A point (x, y, z) is said to be at infinity if z is zero. The line $z=0$ constituted by points at infinity is called the line at infinity.

Teachers will find it necessary to bridge the gap from geometry 2 or 3 to geometry 4 or 5, by some such considerations as those of points at a great distance away on a line, or lines nearly parallel. It is not necessary here to enter into details.

The homogeneous geometries contain two classes of points, those for which z is not zero, which are analogous to points of the earlier geometries, and those at infinity which have no such analogues. This should be, for the student, an interesting parallel to the situation which arises in algebra. There, complex

numbers, $a+bi$, are defined, and some of them, namely those of the form $a+0i$, have properties very like those of real numbers, while the others have no analogues in real algebra.

The teaching of complex algebra must of course precede that of complex geometry. Actually complex algebra is probably taught to all who specialise in mathematics, whereas complex geometry is almost universally neglected. Unless indeed the teacher is prepared to develop his own theory of complex geometry, it is impossible for the subject to be taught, since the text-books make no serious attempt to deal with it. It is true that, in Prof. Neville's *Prolegomena to Analytical Geometry*, there is available for the teacher an admirable account of the subject, but this is not in a form in which it can be assimilated by ordinary schoolboys.

An asymptote of a curve in "geometry 5" can be defined as a tangent at a point of intersection of the curve with the line at infinity, and it is then true, in that geometry, that a curve of degree n has n asymptotes, provided that the usual conventions are made about multiple intersections, etc. The parabola has two asymptotes coincident with the line at infinity. Why the text-books should have excluded the line at infinity as a possible asymptote must remain a mystery. As Prof. Nunn says, it is too subtle for the raw student, and he might have added that it is too ridiculous for the mature one.

What should be the definition of an asymptote in geometry 1? It is probably enough to provide the beginner with a definition applicable to algebraic curves, but it is well for the teacher to realise that two quite natural definitions may lead to different results. In this connexion the curves

$$y=x+\frac{\sin x}{x} \quad \text{and} \quad y=x+\frac{\sin x^2}{x}$$

are interesting.

In geometry 1, an asymptote cannot be defined as a tangent, but it may be defined as the limit of a tangent. A parabola will then have no asymptotes, since $\lim_{m\to 0}$ of $y=\dfrac{x}{m}+am$ does not exist. The same result arrives with Prof. Nunn's definition.

That there should be different results in different geometries should not surprise the student; he probably knows already that $x^4=1$ has two solutions in one algebra, and four solutions in another.

Teachers will realise that this note contains nothing new, except the implied suggestion that commencing specialists ought to be taught certain matters in geometry that have hitherto been neglected. A. R.

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387. John Butterworth, colloquially known as "Jack o' Ben's," was a handloom fustian weaver, and for many years taught mathematics on Sunday mornings in his own little cottage at Haggate, near Royton. "Though self-taught, he was perhaps the ablest geometer in Lancashire in his own days. He possesses the rare faculty of teaching in a very high degree, and many a subsequently successful student was deeply indebted to him for instruction in the higher branches of mathematics. He was a copious and valued contributor to all the mathematical periodicals of his day—a greatly more extensive branch of literature than it is nowadays. Wolfenden, of Hollinwood, was a mentally rich but materially poor man. In elementary mathematics he was, if possible, superior to Butterworth. For many years he calculated the *Liverpool Tide Tables*.—*Memoirs of Morgan Brierley* (1900), p. 18.

388. So well versed did Bossut become in d'Alembert's works, that d'Alembert was accustomed to send those who asked him for explanation to Bossut, as Newton did to De Moivre.—De Morgan, *Penny Cyclopaedia.*

MATHEMATICAL NOTES.

847. [L[1]. 1. c.] *The Equivalence of Pascal's Theorem and Carnot's Theorem.*

That Carnot's theorem and Pascal's are essentially equivalent is obvious, since each of them is a necessary and sufficient condition for six points to lie on a conic; that the passage between them is perfectly direct seems commonly to be ignored. In most of our text-books on pure geometry, Pascal's theorem appears first as the great reward for mastering the idea of a cross-ratio. In a small minority the theorem is derived by projection from the case of a hexagon with opposite sides parallel inscribed in a circle. In neither treatment is the theorem related to the focus and directrix definition except by way of a theory which only the mathematical specialist attacks, and the student whose course of geometrical conics is limited to Euclidean deductions from the focus and directrix definition, as it is under the commonest option for the London pass B.Sc. degree, may very easily fail to make the acquaintance of this most fascinating of geometrical theorems.

To avoid this deplorable result is very easy, and has been so ever since the reintroduction by Taylor of Boscovich's eccentric circle. As everyone knows, the use of this circle leads at once to Newton's theorem, and by a triple application of Newton's theorem we have Carnot's theorem, that if a conic cuts the sides BC, CA, AB of a triangle in Q_1 and R_1, in R_2 and P_2, and in P_3 and Q_3, then

$$\frac{BQ_1 \,.\, BR_1}{CQ_1 \,.\, CR_1} \cdot \frac{CR_2 \,.\, CP_2}{AR_2 \,.\, AP_2} \cdot \frac{AP_3 \,.\, AQ_3}{BP_3 \,.\, BQ_3} = 1.$$

But if P_2P_3, Q_3Q_1, R_1R_2 cut BC, CA, AB in P, Q, R, then by the theorem of Menelaus

$$\frac{BP}{PC} \cdot \frac{CP_2}{P_2A} \cdot \frac{AP_3}{P_3B} = -1,$$

and by multiplication of the three results of this form we see that identically

$$\frac{BP}{PC} \cdot \frac{CQ}{QA} \cdot \frac{AR}{RB} \times \frac{BQ_1 \,.\, BR_1}{CQ_1 \,.\, CR_1} \cdot \frac{CR_2 \,.\, CP_2}{AR_2 \,.\, AP_2} \cdot \frac{AP_3 \,.\, AQ_3}{BP_3 \,.\, BQ_3} = -1.$$

It follows that Carnot's theorem is equivalent to

$$\frac{BP}{PC} \cdot \frac{CQ}{QA} \cdot \frac{AR}{RB} = -1,$$

which is Pascal's theorem.

I would not suggest for a moment that this line of argument goes to the heart of the subject as does the common proof, but I urge that when we are teaching conics, whether at school or at college, to students who we know are not likely ever to touch cross-ratios, we do them serious wrong if we do not lead them by some such track as this to a point from which they can catch a glimpse of descriptive properties. Even the scholarship candidate, admonished that "in order to remember the proof by Chasles' theorem, he should note that the vertices of the two pencils employed are two vertices of the hexagon, separated by *one* vertex," may see some merits in a proof that is unforgettably straightforward.

The equivalence of the two theorems may be utilised in another way if the conic is defined as the projection of a circle. Carnot's theorem is obviously true for a circle, but it is not obviously projective. Having proved Pascal's theorem from Carnot's for the circle, we can legitimately infer Pascal's theorem for the conic by projection, and then return to Carnot's theorem for the conic.

E. H. N.

848. [L¹. 10. d.] *Miquel's Theorem.*

Can any reader tell me why in introducing his own proof of this theorem, Clifford * says, " M. Miquel's proof, reproduced by Catalan, depends on the fact that the circle circumscribing the triangle formed by three tangents to a parabola passes through the focus " ? Clifford's proof belongs to this order of ideas, but there is no mention of a parabola in Miquel's proof in Liouville†, or in two editions of Catalan which I have been able to consult; this proof uses only the elementary Euclidean geometry of the circle.

Since there is no reference to the matter by Coolidge, it seems that writers on the subject have overlooked the effect of inverting the Clifford-Miquel configuration of any order about an arbitrary point. The lines of course become circles, and the whole figure becomes homogeneous; although the circles enter the construction at stages further and further removed from the concurrent circles which replace the lines of Clifford's figure, ultimately they are all on the same footing. The figure consists of 2^n circles concurrent in 2^n points in such a way that $n+1$ circles pass through each of the points and $n+1$ of the points are on each of the circles, and the whole figure can be reconstructed from the circles through any one of its points. Verification is easy, and this form of the theorem has suggested a proof which I have communicated to the *Journal of the Indian Math. Soc.* E. H. N.

849. [D. 1. a.] *Theorems on Limits.*

There is a method of proving certain fundamental limit theorems, avoiding the assumption even of the Binomial Theorem for positive integral exponent, which I think is an improvement on current text-book methods; as thus:

$$\{(1+a)^n-1\}/\{(1+a)-1\}=1+(1+a)+(1+a)^2\ldots+(1+a)^{n-1};$$

$$\therefore\ (1+a)^n=1+a\{1+(1+a)+(1+a)^2+\ldots+(1+a)^{n-1}\}. \qquad (1)$$

Hence, if $a>0$, $$(1+a)^n>1+na. \qquad (2)$$

Apply this inequation to each term in the brackets on the right of (1), and we have

$$\begin{aligned}(1+a)^n&>1+a\{1+(1+a)+(1+2a)+\ldots+1+(n-1)a\}\\&>1+na+(1+2+\ldots+n-1)a^2\\&>1+na+\frac{n(n-1)}{1.2}a^2. \qquad (3)\end{aligned}$$

Application of (2) *to prove* $\lim\limits_{n\to\infty} x^n=0$, *if* $|x|<1$.

$|x|^n$ may be written $\dfrac{1}{(1+a)^n}$, where $a>0$;

$$\therefore\ |x|^n<\frac{1}{1+na},\text{ which}\to 0,\text{ for } n\to\infty;$$

$$\therefore\ |x|^n\to 0;\quad \therefore\ x^n\to 0,\text{ for } n\to\infty.$$

Application of (3) *to prove* $\lim\limits_{n\to\infty}(nx^n)=0$, *if* $|x|<1$.

$$\begin{aligned}n|x|^n=\frac{n}{(1+a)^n}&<\frac{n}{1+na+\dfrac{n(n-1)}{1.2}a^2}\\&<\frac{2}{\dfrac{2}{n}+2a+(n-1)a^2}\\&<\frac{2}{(n-1)a^2}.\end{aligned}$$

* *Messenger*, 5, 124; *Math. Papers*, 38. † 3,485; 10,349.

Now, if ϵ be an arbitrary positive, and $\dfrac{2}{\epsilon a^2}=N+\beta$, where N is integral and β a proper fraction, for values of $n>N+2$ we have

$$n\,|\,x\,|^n<\frac{2\epsilon}{(N+1)a^2\epsilon}<\frac{(N+\beta)}{N+1}\epsilon<\epsilon.$$

Hence $n\,|\,x\,|^n\to 0$ for $n\to\infty$.

Formula (3) can be generalised, to prove that if n is a positive integer,

$$(1+a)^n>1+na+\frac{n(n-1)}{1\,.\,2}a^2+\ldots+\frac{n(n-1)\ldots(n-r+1)}{r\,!}a^r, \text{ if } n>r.$$

This suggests that the above method might yield a new proof of the Binomial Theorem for a positive integral index.

In fact, writing β for $1+a$, (1) may be written

$$\beta^n=1+a(1+\beta+\beta^2+\ldots+\beta^{n-1}),$$

from which, by the aid of mathematical induction, we can prove that

$$\beta^n=1+\binom{n}{1}a+\binom{n}{2}a^2+\ldots+\binom{n}{p}a^p$$
$$+a^{p+1}\left\{\binom{n-1}{p}+\binom{n-2}{p}\beta+\binom{n-3}{p}\beta^2+\ldots+\binom{p}{p}\beta^{n-p-1}\right\},$$

where $\binom{n}{r}$ denotes the number of combinations of n things taken r at a time.

This formula includes the Binomial Theorem as a special case $(p=n-1)$.

R. F. M.

850. [A. 3. K.] *Maxima and Minima of $f(x, y)$.*

In a previous note (*Math. Gazette*, July 1924) a method was given for examining $f(x, y)$ near a stationary value by comparing the orders of smallness of the increments of x and y. This method may be extended as follows.

If at a point where $f_x=f_y=0$ the terms of second degree in h, k in

$$\Delta f\equiv f(x+h, y+k)-f(x, y)$$

are not all 0, then for a maximum or minimum one at least of the coefficients of h^2, k^2, say that of h^2, $\neq 0$. Then Δf may be arranged,

$$\Delta f=ah^2+bhk^m+ck^n+\text{terms of higher order},$$

a, b, c being constants.

Taking k of order 1, h of orders greater or less than m or $\frac{1}{2}n$, we have:

(i) If $n>2m$, there is no max. or min.

(ii) If $n<2m$, there is no max. or min. if n is odd; if n is even, there is a max. or min. or not according as a, c have the same or opposite signs.

(iii) If $n=2m$, there is a max. or min. if $b^2<4ac$;
there is no max. or min. if $b^2>4ac$;

and if $b^2=4ac$, we put $ah+\frac{1}{2}bk^m=H$, and examine similarly the resulting expression in H, k.

If the terms of second degree are all 0, then if the non-vanishing terms of lowest degree contain any real simple linear factors, there is no max. or min., and there is a max. or a min. if the linear factors are all complex; while if there are real repeated linear factors, either case may arise.

In cases where these terms may be factorized, the expression may be easily examined by the foregoing method. Suppose, for instance, the first non-vanishing terms are of degree 4 (obviously the degree must be even for a max. or min.), and there is a real repeated factor $ah-bk$. By the substitution $ah-bk=H$ we obtain

$$\Delta f=H^2f_2(H, k)+f_5(H, k)+\ldots,$$

f_2 being of degree 2 in H, k, and supposedly of fixed sign.

Thus $\Delta f = H^2 f_2(H, k) + bHk^m + ck^n +$ terms of higher order.

For a max. or min. n must be even, $=2p$ suppose.

If $H = O(k)$, there is no change of sign in Δf.

If $H = o(k)$, the leading terms are obtained from

$$aH^2k^2 + bHk^m + ck^{2p}.$$

Try H of orders greater or less than m, $p-1$.

We have a max. or min. only if $p-1 < m$ and a, c have the same sign, or if $p-1=m$ and $b^2 < 4ac$. If $p-1=m$ and $b^2=4ac$, we put $aH + \frac{1}{2}bk^{p-1} = H'$ and re-examine the expression. H. V. MALLISON.

851. [A. 3.] *Difference Equations.*

I have tried the experiment of teaching boys, who are in the second year of their advanced course, the solution of difference equations with constant coefficients, and using the method for recurring series, recurring continued fractions, and so on. The boys did not find any difficulty at all, not even with repeated or imaginary roots in the auxiliary equation, while the time saved was considerable.

Ex. 1. Find the nth term of the recurring series, 1, 2, 5, 14.

We have $5 = 2a + b$, $14 = 5a + 2b$, whence $a = 4$, $b = -3$. We have then to solve the equation $u_n = 4u_{n-1} - 3u_{n-2}$. Putting $u_n = x^n$ as a trial solution we get $x^2 - 4x + 3 = 0$, so that $x = 1$ or 3 and $u_n = A + B \cdot 3^n$, where $1 = A + 3B$ and $2 = A + 9B$. Hence $u_n = \frac{1}{2}(1 + 3^{n-1})$.

Ex. 2. Find the nth convergent to $1 + \frac{1}{2+} \frac{1}{2+} \frac{1}{2+} \dots$.

The 1st two convergents are 1/1 and 3/2.

If u_n denotes p_n or q_n, $u_n = 2u_{n-1} + u_{n-2}$.

$$\therefore\ x^2 = 2x + 1 \quad \text{and} \quad x = 1 \pm \sqrt{2};$$

$$\therefore\ u_n = A(1+\sqrt{2})^n + B(1-\sqrt{2})^n.$$

For p_n, $A = B = \frac{1}{2}$; and for q_n, $A = -B = \dfrac{1}{2\sqrt{2}}$;

$$\therefore\ \frac{p_n}{q_n} = \frac{(1+\sqrt{2})^n + (1-\sqrt{2})^n}{(1+\sqrt{2})^n - (1-\sqrt{2})^n} \cdot \sqrt{2}.$$

Ex. 3. The first two terms of a series are given, and every succeeding term is the arithmetic mean of the two preceding terms. Find the nth term.

Here $2u_n = u_{n-1} + u_{n-2}$, $2x^2 - x - 1 = 0$, $x = 1$ or $-\frac{1}{2}$;

$$\therefore\ u_n = A + B(-\tfrac{1}{2})^n, \text{ etc.}$$

Ex. 4. The first three terms of a series are given, and every succeeding term is the arithmetic mean of the three preceding terms. Find the nth term.

Here $$3u_n = u_{n-1} + u_{n-2} + u_{n-3};$$

$$\therefore\ 3x^3 = x^2 + x + 1;$$

$$\therefore\ x = 1 \quad \text{or} \quad (-1 \pm i\sqrt{2})/3.$$

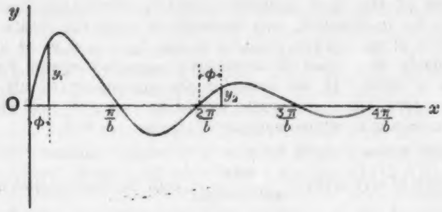
Hence $$u_n = A + r^n r' \cos(n\alpha + \alpha'),$$

where $r \cos \alpha = -\frac{1}{3}$, $r \sin \alpha = \dfrac{\sqrt{2}}{3}$ and A, r' and α' have to be found from the first three terms. N. M. GIBBINS.

852. [V. 1. a. λ.] *Note on the geometry of the curve* $y = e^{-ax} \sin bx$.

The properties of the function $y = e^{-ax} \sin bx$ are so important in electricity and elsewhere, that any convenient method of forming a mental picture of them will be most helpful. The following has occurred to me as being a simple

method of deducing the periodic properties from a knowledge of those of the first period.



The period $=2\pi/b$.

Let y_1, y_2 be ordinates for values of $x=\phi$ and $\phi+2\pi/b$, which we may call corresponding ordinates, by which we mean ordinates occupying the same relative positions within succeeding periods. Then

$$\frac{y_2}{y_1}=\frac{e^{-a(\phi+2\pi/b)}\sin b(\phi+2\pi/b)}{e^{-a\phi}\sin b\phi}=e^{-2a\pi/b},$$

an expression which does not involve ϕ.

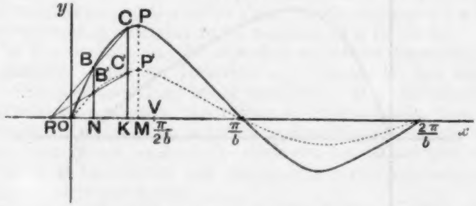
This means that ordinates in the second period can be got from corresponding ordinates in the first period by multiplying by a constant ratio $e^{-2a\pi/b}$. This follows also from the fundamental property of the function $y=e^{-ax}$, that equi-spaced ordinates form a geometrical progression; for if we consider ordinates at intervals of h, they form the G.P., $e^{-ah}, e^{-2ah}, e^{-3ah}\dots$, the common ratio being e^{-ah}.

If we choose h to be $2\pi/b$, the period of $\sin bx$, the common ratio of the G.P. becomes $e^{-2a\pi/b}$. Hence we have the following:

Ordinates of e^{-ax} at intervals of $2\pi/b$ form a G.P. of common ratio $e^{-2a\pi/b}$.
But „ „ $\sin bx$ „ „ $2\pi/b$ are equal;
$\therefore$ „ „ $e^{-ax}\sin bx$ „ „ $2\pi/b$ form a G.P. of common ratio $e^{-2a\pi/b}$.

If illustrated by means of a diagram, this is easily understood by elementary pupils, and the nature of the function e^{-ax} is clearly realised.

We can now apply the second period of the graph of $e^{-ax}\sin bx$ to the first, as shown in the diagram, the second period being shown dotted.



Then the second period can be got from the first by straining every ordinate in the constant ratio $e^{-2a\pi/b}$. Similarly the third period can be got from the second, and so on. Hence the following properties at once follow:

(1) The maximum ordinate MP strains into the maximum ordinate MP' $(=MPe^{-2a\pi/b})$, and successive maxima form a G.P. whose common ratio is $e^{-2a\pi/b}$. Also successive maxima occupy the same relative position in their respective periods, for MV is the same for all. But it should be remembered

that this is but part of the general property that corresponding ordinates form a G.P. of common ratio $e^{-2a\pi/b}$.

(2) (The area of the 2nd positive crest) $=e^{-2a\pi/b}$ (the area of the first positive crest), for in straining any element of area the width is unaltered. Hence the areas of successive positive crests form a G.P. of common ratio $e^{-2a\pi/b}$. Similarly the areas of successive negative crests form a G.P. of common ratio $e^{-2a\pi/b}$. If we consider the succession of all crests, either positive or negative, the strain ratio would be $-e^{-a\pi/b}$, and the areas of all crests would form a G.P. whose common ratio is $-e^{-a\pi/b}$.

(3) Successive mean values form a G.P. whose common ratio $=-e^{-a\pi/b}$, since mean value of any crest $=\dfrac{\text{area}}{\text{half period}}$, and the half period is the same for each crest.

(4) For the succession of all crests, the successive mean square values form a G.P. whose common ratio $=e^{-2a\pi/b}$, for since

$$\frac{y_2}{y_1}=-e^{-a\pi/b}, \quad \therefore \frac{y_2^2}{y_1^2}=e^{-2a\pi/b}.$$

Since the width of any element is unaltered during strain, it follows that

$$\frac{\int y_2^2\,dx}{\int y_1^2\,dx}=e^{-2a\pi/b},$$

the integration being taken over the appropriate crests. Whence the theorem follows. If k^2 be the M.S.V. for the first crest, the heating effect of the successive crests would be

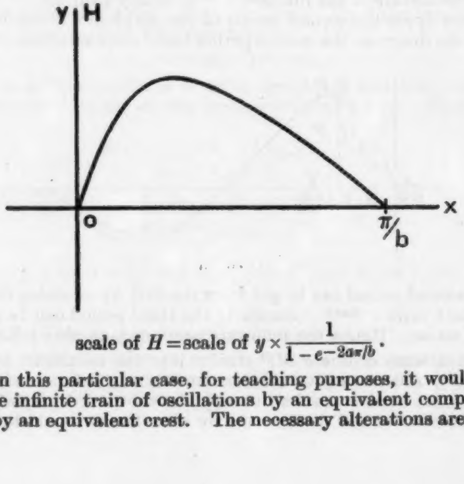
$$\frac{\pi}{b}\cdot k^2;\ \frac{\pi}{b}\cdot k^2\cdot e^{-2a\pi/b};\ \frac{\pi}{b}\cdot k^2\cdot e^{-4a\pi/b}\ \dots,$$

and the heating effect of the whole train of crests would be the sum to infinity of this G.P., *i.e.*

$$\frac{\pi}{b}\cdot\frac{k^2}{1-e^{-2a\pi/b}},$$

when, of course, y denotes the current at time x.

Whence, so far as heating effect goes, we could replace the whole train of crests by the first crest, and simply alter the scale of y in the ratio $1:\dfrac{1}{1-e^{-2a\pi/b}}$.



Thus scale of $H=$ scale of $y\times\dfrac{1}{1-e^{-2a\pi/b}}$.

Probably in this particular case, for teaching purposes, it would be better to replace the infinite train of oscillations by an equivalent complete period, rather than by an equivalent crest. The necessary alterations are simple, the

succession of M.S.V.'s then forming a G.P. of common ratio $e^{-4a\pi/b}$, and k^2 then applying to the first complete period.

(5) The following geometrical construction enables us to construct any crest after the first has been drawn. The two crests, as drawn, are in perspective with the centre at infinity, and x-axis as axis of perspectivity. Therefore the joins of corresponding points meet on Ox. Let C and C' be a pair of corresponding points on the 1st and 2nd crests. These might be chosen at the quarter-period. Let B be any other point on the 1st crest. Let CB meet Ox in R. Let RC' meet NB, the perpendicular to Ox from B, in B'. Then B' is the point on the 2nd crest, which corresponds to B on the 1st crest. The construction also follows simply by considering similar $\triangle^s\, ONBB'$, $OKCC'$.

I find that pupils are inclined to regard the fact that successive max. and min. form a G.P., etc., as being a peculiar property of the function $e^{-ax}\sin bx$. Rather should we regard e^{-ax} in the nature of an operator which, when operating on any periodic function of x, strains successive periods (and not merely the max. and min. ordinates of those periods) in a constant ratio.

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853. [C. 2. h.]

Can any reader give an elementary verification of the results:

$$\int_0^{\frac{\pi}{2}} \sec^{-1}(\sec x+2)\,dx = \frac{5\pi^2}{24},$$

$$\int_0^{\frac{\pi}{3}} \sec^{-1}(2\cos x+1)\,dx = \frac{\pi^2}{8},$$

$$\int_0^{\frac{\pi}{3}} \cos^{-1}\tfrac{1}{2}(\sec x-1)\,dx = \frac{11\pi^2}{72},$$

which have been suggested by a geometrical consideration and verified graphically?

D. COXETER (*per* A. R.).

854. [X. 8.] As new apparatus for teaching mathematics is not very common, may I direct attention to my patent No. 16326, of 29th June, 1926.

This apparatus, which I call a "Dial Machine," consists of a number of dials, each with a pointer, fixed on one side of a board, the readings being connected by concealed mechanism on the other side. Each dial carries a conspicuous letter for convenience in referring to its readings in a formula.

The task for the student may be regarded as that of describing a tangle of functional relations which he discovers experimentally, the nature of the mechanism which produces them being unknown. The system of dials is thus rather like a miniature physical laboratory of direct-reading instruments.

The mechanism is designed so as not to be complicated, and to be so adjustable that the connections between the dials can be shifted and regrouped at will. The types of connection are one-to-one correspondence, one-to-two, and one-to-three correspondence.

The apparatus is intended to promote the fusion or alternative use of algebraic and geometric language, since it is natural to call the dials dimensions, all the readings of two dials a space of two dimensions, all the readings of four dials a space of four dimensions, and so on. Similarly, a one-to-one correspondence between two dials is called a line, a one-to-two correspondence between three dials a surface, and so on. The dials are at right angles by definition of right angle.

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855. [L^1. 9. d.] ***Determination of the Ratio of Arc of an Ellipse to Major Axis.***

If a sector of a circle of radius l having angle $2\pi n^c$ is folded to form whole surface of a right circular cone, the radius of the base of cone is ln.

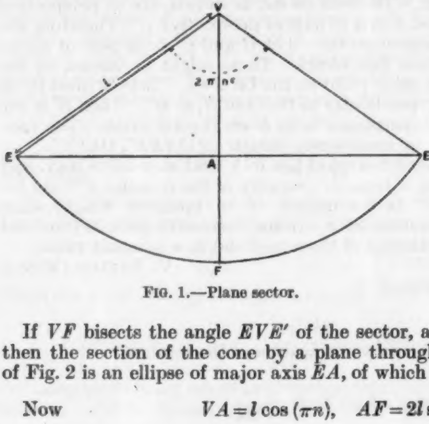

FIG. 1.—Plane sector.

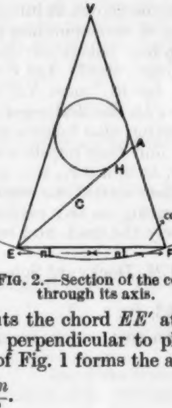

FIG. 2.—Section of the cone through its axis.

If VF bisects the angle EVE' of the sector, and cuts the chord EE' at A, then the section of the cone by a plane through EA perpendicular to plane of Fig. 2 is an ellipse of major axis EA, of which EE' of Fig. 1 forms the arc.

Now $$VA = l\cos(\pi n), \quad AF = 2l\sin^2\frac{\pi n}{2}.$$

$$AE^2 = (2nl)^2 + l^2(1-\cos n\pi)^2 - 4nl^2 \times n(1-\cos n\pi)$$
$$= l^2(1 - 2\cos \overline{n\pi} + \cos^2 n\pi + 4n^2\cos n\pi);$$
$$\therefore\ (2a)^2 = 4l^2\left(\sin^4\frac{n\pi}{2} + n^2\cos\overline{n\pi}\right)$$
$$= 4l^2 . S^2,$$

and $$\text{arc} = 2l\sin(\pi n) = \frac{2a\sin n\pi}{S}\quad\left[\text{where } {}^*S^2 = \sin^4\frac{n\pi}{2} + n^2\cos n\pi\right].$$

If the inscribed $\odot$ of $\triangle AVE$ touches AE at H, and C bisects AE,

$$2CH = EH - AH = VE - VA = l - l.\cos\overline{\pi n} = 2l\sin^2\frac{n\pi}{2};$$

$$\therefore\ e = \frac{l}{a}\sin^2\frac{n\pi}{2} = \frac{\sin^2\frac{n\pi}{2}}{S}.$$

Examples. $n=\frac{1}{2}$, $S=\frac{1}{2}$, $e=1$, EVE' is a semicircle, and the ellipse becomes a st. line from E to V and back.

$n=\frac{1}{4}$, $S=\cdot256$, $e=\cdot572$, $\dfrac{\text{arc}}{2a}=2\cdot75$,

$n=\cdot3$, $S=\cdot309$, $e=\cdot67$, $\dfrac{\text{arc}}{2a}=2\cdot62$

$n=\frac{1}{3}$, $S=\dfrac{\sqrt{17}}{12}=\cdot343$, $e=\dfrac{3}{\sqrt{17}}=\cdot728$, $\dfrac{\text{arc}}{2a}=\dfrac{6\sqrt{3}}{\sqrt{17}}=2\cdot52.$

$n=\cdot4$, $S=\cdot411$, $e=\cdot84$, $\dfrac{\text{arc}}{2a}=2\cdot31$

Malvern College. L. S. MILWARD.

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* S can be arranged as $\sqrt{\left(\sin^2\frac{n\pi}{2}-n^2\right)^2+n^2(1-n^2)}$.

REVIEWS.

The Origin, Nature, and Influence of Relativity. By G. D. BIRKHOFF. Pp. 185. 8s. 6d. net. 1926. (Macmillan Co., New York).

This book represents some lectures on Relativity which Professor Birkhoff gave recently, and which he has been persuaded to write out for publication. After leaving this explanation of its origin and seeing its slim proportions, the reader may jump to the conclusion that it is one of the innumerable volumes in which the ideas of Einstein are dilated and expounded, with diagrams and analogies and paradoxes, for the benefit of the man in the street. It is, however, nothing of the kind : it is a most suggestive and valuable essay, full of original ideas, and with very little that has been borrowed from anywhere else. The parts which are merely exposition of Einstein are neither very extensive nor (to be quite frank) very well done : the English style is in places obscure, and one has now and then to stop and puzzle out the meaning and connexion of the sentences. But this fault is more than redeemed by the freshness of outlook, the novelty of treatment, and the rich vein of ideas which crops out continually, and which a lesser man would have written up as a dozen research papers.

As Professor Birkhoff points out, it is idealised interstellar space which furnishes the *motif* for the new physics, much as the idealised rigid body did for the old. In interstellar space are found vast stretches of emptiness with here and there comparatively infinitesimal stars, which may be idealised as particles moving relatively to one another with velocities mostly of the order of 20 miles a second, and radiating light in every direction. In this new model the primary measurable quantity is duration at any one of the particles, and this duration may be supposed to be measurable by means of clocks carried by the particles. Starting with the clocks, the light-signals from one particle to another, and the assumption that space-time is alike in all its parts, we are led almost inevitably to the formulae of relativity.

The most important parts of the book are Chapter VI. (" Some General Principles of the New Physics ") and VII. (" The Structure of Matter in the New Physics.") The former of these is a sketch of a complete system of axiomatics for Relavitity, which may be compared with other systems of axiomatics published in recent years, *e.g.* that of Reicherbach (*Phys. Zeits.*, 1921, and *Zeits. f. Phys.*, 1925) and that of Carathéodory (*Berlin Sitzungsb.*, 1924). Professor Birkhoff bases the theory on eight postulates: the postulates (1) of the Space-Time Continuum, (2) of the Field, (3) of Causation, (4) of Isometry, (5) of Local Time, (6) of Equivalence, (7) of the Gravitational Field, and (8) of Stability. His discussion throws a flood of new light on the foundations of Physics, and is worthy of the most careful study.

In the following chapter a problem of the greatest importance is attacked—that of the structure of matter and its connexion with electricity. Why does not the electron explode in consequence of the mutual repulsion of its parts? Mie was the first to construct a logical answer to such questions (*Ann. d. Phys.*, 1912 and 1913) by proposing modifications in the field-equations and the energy-tensor of the Maxwell-Lorentz theory. Now Professor Birkhoff undertakes an independent investigation, in which three more postulates are introduced, (9) that of the Perfect Fluid, (10) of the Electromagnetic Field, and (11) of Fluid Tension. The result is a new view of the rationale of atomic structure, leading to the hydrogen and helium atoms. It seems possible that radiationless " stationary states " may be accounted for on these hypotheses, and that the whole aggregate of quantum phenomena may be satisfactorily explained by a natural development of the classical theory. " As far as I know," says Professor Birkhoff, " this possibility has not been taken sufficiently into account in the past. It should be thoroughly investigated."

The whole chapter is of absorbing interest, but one cannot help being amused at the notion that a popular audience, or indeed any audience, could take it in from an hour's lecture.

Comments on two minor points may be added. On page 9 we read that Pythagoras " seemed to have held secretly the heliocentric theory, which gave

the sun a central place." There is not, so far as I know, any evidence tending to make such a statement probable. The first propounder of the heliocentric system, Aristarchus, lived nearly 300 years after Pythagoras.

On page 107, "at present the astronomical evidence does not point to the finiteness of space." This is a matter of opinion, but there is a remarkable degree of agreement between the estimates which have been obtained, in the last two or three years, from different sets of astronomical data, for the radius of curvature of the space-time universe: practically all of them are in the neighbourhood of 10^{13} astronomical units.

E. T. WHITTAKER.

The Quantum Theory of the Atom. By G. BIRTWISTLE. Pp. xi + 233. 15s. net. 1926. (Cambridge University Press.)

There is always room for new books which present an up-to-date and orderly account of a subject like the Quantum Theory which is in course of rapid development. Mr. Birtwistle's book is especially welcome in that he has preferred to be concise rather than encyclopaedic, while at the same time nothing essential to the ground he covers has been omitted. The leading feature of the work is undoubtedly the care and lucidity with which the mathematical developments are carried through; the fundamental assumptions such as the "frequency condition" and the "adiabatic principle" are stated clearly and shortly, and their implications are developed mathematically without further ado. While thus its spirit is mathematical throughout, the work should prove very welcome to physicists, because of the ease and clearness with which such readers are led through that disagreeable but necessary adjunct of their subject, its mathematics.

The book opens with an account of the origin of the Quantum Theory in the attempt to explain the phenomena of radiation. A short chapter on the photo-electric effect and on Einstein's deduction of the radiation formula is also included, apparently on historical grounds, as it is not germane to the author's real theme, the theory of spectra and of atomic structure. On this topic the main questions dealt with are the spectra of hydrogen-like atoms, the relativity, Stark and Zeeman effects on such spectra, the elementary theory of the multiple structure of spectral lines, and X-ray, absorption and band spectra. A specially welcome feature is the inclusion of the relevant parts of the Transformation and Perturbation Theories of General Dynamics. It is to be hoped that the reception of this work may encourage the author to give us a second volume dealing with such topics as complex spectra, the "anomalous" Zeeman effect, the spinning electron and the new Quantum Mechanics.

While, as already stated, the mathematics is for the most part admirably clear, isolated sections are open to criticism. In § 49, where it is desired to prove that the coordinates q are periodic functions of the angle-variables w, it is implicitly assumed that the q's are *single-valued* functions of the w's; this is a serious gap in the argument, which always appears to be passed over in silence, as, for example, by Sommerfeld. The treatment of degenerate systems in § 55 misses the essential point, for it is not explained why the relation

$$\kappa_1\omega_1 + \kappa_2\omega_2 + \ldots + \kappa_s\omega_s = 0$$

is of significance only when the κ's are integers. In § 58, on the adiabatic invariance of the quantum integrals, there are some mathematical errors of an elementary nature; it would also be appropriate here to make some mention of the fact that Burgers' argument requires modification on the lines indicated by Dirac and Thomas. In a few other places there are minor inaccuracies.

T. M. C.

Matrices and Determinoids. By C. E. CULLIS. Pp. xviii + 681. Vol. III. Part I. 63s. net. 1925. (Cambridge.)

This is a continuation of the work on matrices which the author originally gave as Readership Lectures in the University of Calcutta. The first volume was published in 1913; the second in 1918. The present volume contains actually only part of the matter first intended for it, so that we still await the completion of the whole thesis, which is a systematic account, in consecutive and very complete form, of a rather neglected field of algebra.

Until the next part appears, it is impossible to do justice to the work as a whole and to say how far it is likely to influence the mathematical world. In our present position we are viewing the erection of a massive and stately building, several buttresses and wings of which are completely fashioned. It exhibits a breadth of architectural insight, and the treatment shows dignity and ease in handling none too tractable a material.

Before coming to details it is well to explain as shortly as may be what matrices and determinoids are. A matrix is simply a set of numbers co-ordinated together in a square or oblong formation. Let us write

$$A = \begin{bmatrix} 1, & 2, & 3 \\ 4, & 5, & 6 \\ 7, & 8, & 9 \end{bmatrix}, \qquad B = \begin{bmatrix} 1, & 4, & 7 \\ 2, & 5, & 8 \\ 3, & 6, & 9 \end{bmatrix}$$

for illustrations. Then each of A and B is a matrix of order three. Were there less rows than columns (or *vice versa*) the order would be specified by two numbers. Thus

$$C = \begin{bmatrix} 1, & 2, & 3 \\ 4, & 5, & 6 \end{bmatrix}$$

is a matrix of orders two and three. The general matrix has orders m and n.

Now we are all familiar with the idea of the matrix, for ordinary Cartesian coordinates

$$[x, y]$$

of a point P in a plane provide a simple instance. Here the matrix is of orders one and two. This involves more than merely the two numbers x and y; it is two numbers together with a specific relation between them, namely that x precedes y. The geometrical interpretation of $[x, y]$, the point P, is not the same as that of $[y, x]$; and this is hint enough that, regarded as algebraic elements (molecules if we like), we may with advantage study the behaviour of matrices, always treating them as single integral things, and not as elaborate clusters of component parts. We have to thank Cayley for first enunciating this principle. He, however, confined the definition of matrix to a square formation only.

So unless $x = y$, we distinguish between

$$[x, y] \text{ and } [y, x]$$

as the geometrical interpretation certainly suggests. But it must be borne in mind that the theory of matrices is entirely algebraic, and is logically built up from specific foundations of definitions and primitive propositions. The geometry suggests certain properties, though it does not help us much here except for one-rowed matrices; but, as Professor Cullis rightly points out, the whole subject is a piece of algebra with a value of its own, suggested in the first instance by geometry, but growing and developing in its own right; and maybe, in its fruition, giving back gifts of rich import to geometry, and even to other great fields of learning.

If we write

$$|A| = \begin{vmatrix} 1, & 2, & 3 \\ 4, & 5, & 6 \\ 7, & 8, & 9 \end{vmatrix}$$

we have at once the determinant of the matrix A. The determinant and the matrix are quite distinct, as distinct in fact as the area of a triangle and a triangle itself. The one is a numerical function of the other. In the above example A and B are different matrices, although their determinants are equal. Technically B is called the transposed of A. Two matrices are equal only when every corresponding element of one is equal to that of the other. For example,

$$\begin{bmatrix} x+x, & 2, & 5 \\ 4, & y, & x+y \end{bmatrix}$$

is equal to

$$\begin{bmatrix} 2x, & 1+1, & 2+3 \\ 4, & y, & x+y \end{bmatrix}.$$

A *determinoid* corresponds to an oblong matrix as a determinant to a square matrix. It is relatively unimportant in the present volume.

If a_{ij} and b_{ij} are corresponding elements of A and B, the sum of A and B is a matrix with $a_{ij}+b_{ij}$ for corresponding element. Obviously the orders of the matrices must be conformable. It is therefore agreed to fill up the array which wants rows or columns with zeros. This filling up process takes place below or to the right of what is given. In the examples cited

$$A+B=\begin{bmatrix} 2, & 6, & 10 \\ 6, & 10, & 14 \\ 10, & 14, & 18 \end{bmatrix},$$

whereas

$$A+C=\begin{bmatrix} 2, & 4, & 6 \\ 8, & 10, & 12 \\ 7, & 8, & 9 \end{bmatrix},$$

obtained by first affixing a row of zeros to C to make it conform with A.

Cayley gave the rules for adding, subtracting and multiplying matrices. They lead to such results as

$$A+B=B+A,$$
$$A+(B+C)=(A+B)+C,$$
$$A(B+C)=AB+AC,$$

but AB is not generally equal to BA.

Here the product AB is formed as in multiplying determinants, by weaving the rows of A into columns of B. Still using our numerical illustrations, this gives

$$AB=\begin{bmatrix} 14, & 32, & 50 \\ 32, & 77, & 122 \\ 50, & 122, & 194 \end{bmatrix},$$

but

$$BA=\begin{bmatrix} 66, & 78, & 90 \\ 78, & 93, & 108 \\ 90, & 108, & 126 \end{bmatrix}.$$

The importance of correctly learning how to multiply matrices suggests that in teaching the product rule of determinants this row and columnwise method should habitually be used. Here, for instance, in AB the first entry 14 is formed from the first row and first column of A and B respectively.

The most important feature about a matrix is its *rank*, which is a whole number not exceeding either of its orders, and possibly being zero. Rank plays for matrices the part that degrees of freedom play in geometry, the connection being now in *homogeneous* coordinates. Interpret A, above, as three points whose areal coordinates are given by rows [1, 2, 3], [4, 5, 6], [7, 8, 9]. Geometrically these particular points are restricted by one condition; they are in line. Algebraically this gives to matrix A a *nullity* one and *rank* two. Nullity, which is Sylvester's word, is complementary to rank, the sum of the two forming the order of a square matrix. Now, if instead we have rows [1, 2, 3], [4, 8, 12], [− 2, − 4, − 6], the resulting matrix has rank *one*. Geometrically in this case we are never getting away from the same one point. Again [0, 0, 0], [0, 0, 0], [0, 0, 0] leads to a matrix of rank zero. Geometrically it gives no point at all, for homogeneous coordinates. Thus no point, a point, a line, a triangle of points correspond to a matrix of rank 0, 1, 2, 3 respectively.

All this explanation gives the bare elements of the theory which has grown up round these ideas. The origins of it, some seventy years ago, are due to three of our countrymen, Cayley, Sylvester and H. J. S. Smith. As so often happens with discoveries in England, they have to go abroad to take effect. Thus Smith is now almost forgotten as the pioneer in the theory of invariant factors (*elementarteiler*), called in the present book *potent divisors*: his work was so enlarged by Weierstrass, and others. Also the main lines of the theory, laid by Cayley and Sylvester, were enormously improved by the use which Frobenius made of them for his theory of bilinear forms. Sylvester in fact foresaw great things for this algebra, and really inspired Professor Cullis to fulfil his programme. Thus in the present volumes a real service is being done for mathematicians, by exhibiting the importance of matrix theory in all its breadth and detail.

It must be confessed, however, that the volume would be vastly more entertaining if the author had shown his hand a little more freely and told us

a little more of the history of his subject. The book is as bare of references and historical remarks as a work of art is bare of its skeleton design. This does not suit all mathematical readers, and certainly not those who like to attain a true perspective in their reading. This lack here may probably be set down to the modesty of the author who does not wish to show too explicitly how much of the book is his own thought and research working on the existing raw material. The omission, too, is quite intentional, as is stated in the preface, which incidentally is very good reading, and shows what a thorough feeling for algebra the author has.

The volume is broadly in four divisions. The first two or three chapters are a monumental account of polynomials in any finite number of variables, in all their relations comprised under such terms as irresoluble forms, factors, eliminants and the like. This alone makes the book a valuable addition to English works on algebra and algebraic geometry. It is, as far as the present reviewer knows, the only really complete account in the English language of these fundamental facts of algebra. Italy and Germany have hitherto been better off.

Next there come the three chief subjects of the book, for which these preliminary chapters were inserted to prepare the way. These are the theories of (i) potent divisors (*elementarteiler*), (ii) commutants, and (iii) invariant transformands. Of these (i) is the theory by which the essential properties of the equation in λ,

$$\begin{vmatrix} a-\lambda, & b, & \dots \\ c, & d-\lambda, & \dots \\ \dots & \dots & \dots \end{vmatrix} = 0,$$

are analyzed. If A denote the square matrix of these elements $a, b, c, d \dots,$ when λ is dropped, and if E is the unit matrix (of units through the leading diagonal and zeros elsewhere), the equation may be written

$$A - \lambda E = 0.$$

To suggest that this is of the nth degree in λ, it may also be written $D_n(\lambda) = 0$. It occurs, when $\lambda = 2$, in the theory of reducing the equation of a conic to its form referred to principal axes. It occurs equally spontaneously in dynamics when there are two or more degrees of freedom in a system. It occurs in geometry most naturally when the fixed points of a collineation of space of $n-1$ dimensions are sought. Yet the key to its full analysis was provided by Smith in his research in the theory of numbers! Arguments which Smith used of positive integers were shown by Kronecker and Weierstrass to be equally valid for more complicated domains of rationality: truly a brilliant enlargement of the original theory. Along with this came the Cayley-Hamiltonian discovery that the equation

$$D_m(\lambda) = 0$$

is not merely solved by ordinary numbers, but is satisfied by the original matrix itself (p. 318). This astounding generalization, round which Chapter XXV. is written, explains the purport of the whole work. Incidentally it justifies the preliminary chapters, for the author now has in mind the study of polynomials, where each coefficient and each variable may be a matrix. These opening chapters with their meticulous discussions of extreme instances, as when there are plenty of zeros about among the coefficients and exponents in a polynomial, read at first like the strange hair-splitting conundrums of the mediaeval schoolmen. They now take on a far-reaching significance when a coefficient may be a matrix, and when it is realized that the vanishing of a product AB does not necessarily imply the vanishing of either factor A or B.

One is so used to thinking of a term in ordinary algebra as a coefficient and a variable factor, or if we prefer to say it, as having *one* coefficient, that one does not readily see why the term should more properly be looked on as a sandwich, with slices of coefficient on each side of the variable. Thus we instinctively would think of $2 \times x \times 3$ as $6x$. Not so in matrices, for if A, B, X are matrices, AXB is not ABX unless X, B are commutative.

The typical term of the rth degree in one variable is

$$AX^rB$$

with its coefficients duly placed fore and aft. This leads very naturally to two fundamental questions. First, if AX is not XA, presumably it is X multiplying something else. What then is the solution of the equation $AX=XB$? This raises the whole theory of *commutants*, the name given to solutions X of this. Secondly, in what circumstances does a typical linear term AXB simplify to X, the term with unit coefficient? This in turn raises the question of *invariant transformands*, the name given to solutions X of the equation $AXB=X$. The last two great sections of the volume are concerned with these very two questions. It may be said that they are very adequately set out and discussed. The solutions are reminiscent of integrals of linear differential equations. They are also poristic, that is to say, they depend for their existence on preliminary conditions holding among the coefficients; and if one non-zero solution exists an infinite number do.

Incidentally the solution of Frobenius' equation

$$X^2=A$$

is discussed. Matrix algebra has in fact got as far as quadratic equations.

The book is not a first course on matrices, but would follow very nicely on such an introductory account as Böcher's *Higher Algebra*, the plan of which it almost completely ingulfs. It requires patient reading, because it is written synthetically, rather like Euclid. Also there is a slow rhythmic adagio in its passage from theme to theme which in these days of hurry is by no means without its charm.

One curious effect of reading certain passages of the book is the revelation it gives of the poverty of human language in the presence of a two-dimensional pattern. Reading and writing are essentially like the one-rowed matrix, in spite of the craze for cross-word puzzles: and the written sentence is sometimes a ridiculously imperfect expression for an elaborate but quite obvious check pattern. The tiresome definition of the class of a compound matrix, on p. 410, which would be obvious with the help of a diagram, is like trying to set Bradshaw to literature.

The book is highly suggestive and may be warmly recommended.

H. W. Turnbull.

Lehrbuch der Ballistik. By Dr. C. Cranz. Vol. I., **External Ballistics.** Fifth edition. Pp. xx + 712. 57 goldmarks. 1925. (Springer, Berlin.)

The new edition of the *Lehrbuch* will consist of three volumes: Vol. I., External Ballistics; Vol. II., Internal Ballistics; Vol. III., Experimental Ballistics. Vol. I. now appears in its fifth edition; Vol. II., which, for various reasons, has not yet been published, is soon to appear; a new edition of Vol. III. will presumably appear later.

The subject of External Ballistics was considerably developed during the war. The demand for longer ranges necessitated the development of step-by-step methods of calculating trajectories; with increased ranges the effects of meteorological and other variations demanded greater attention; the effects of the oscillations of a spinning projectile increased in importance. The accuracy of the German long-range gunnery during their spring offensive in 1918 was largely due to the accurate calculation of corrections for these effects.

In the new edition of Vol. I. these developments are treated and, in some cases, the English, French, American and German methods are compared. In this connection the author has been ably assisted by Major Dr. Becker and Prof. O. von Eberhard, who calculated the trajectories, etc., for the guns which bombarded Paris at a range of over 70 miles.

An unduly large portion of the volume is devoted to early methods of solving the differential equations of motion involving approximate algebraical expressions for the resistance of the air. Such methods are now of little more than historical interest, and the space occupied by them might with advantage be devoted to more detailed accounts of modern methods. The calculation in Chapter II. of the coefficient of shape and of the effect of yaw is based on

assumptions upon which, the author admits, little reliance can be placed. In the circumstances it would appear to be better to omit these calculations and to give experimental results in their place.

The inclusion in the volume of tables relevant to the subject, instead of forming a separate volume of them, is a welcome innovation; for facility of reference it would have been better if paragraph numbers had been printed above the text on each page.

The appearance of Vols. II. and III. will be awaited with eagerness by Ballisticians and Artillerists, and it is hoped that they will fulfil the expectations born of the present volume.

F. R. W. HUNT.

Probabilités Géométriques. By R. DELTHEIL. Fascicule 2 of Vol. II. of the *Théorie des Probabilités* by E. BOREL. Pp. 123. Fr. 26. 1926. (Gauthier-Villars.)

M. Deltheil has written a fascinating account of the application of the theory of probability to geometrical problems. The treatment of the subject is systematic and modern, and the lucidity of the author's exposition leaves nothing to be desired. The problem of the measure of a group receives careful and adequate treatment, and, having established the relevant results of the theories of groups of transformation and integral invariants, the author is in a position to give precision to the choice of the integrals which express the probabilities in the different examples discussed. Among the particular cases examined by the author are the celebrated problem of the needle solved by Buffon, the well-known work of Sylvester on the convex quadrilateral, and the theorems due to Crofton. In dealing with Crofton's work, the author has used the method of functional determinants due to Lebesgue. The book is a valuable addition to M. Borel's treatise, and will be welcomed by those who desire a precise, self-contained, and modern introduction to the subject.

Sur les Probabilités Géométriques. By B. HOSTINSKY. Publications de la Faculté des Sciences de l'Université Masaryk. Pp. 26. N.p. 1926.

The purpose of this monograph is to give several extensions of the theorems of Crofton and Czuber on geometrical probability. From the multiple integrals which express the measure of groups of points, lines, and planes, the author obtains directly the results due to Crofton and Czuber, and proceeds to discuss the following extensions:

(1) The probability that two secant planes of a closed surface cut in a line which is itself a secant of the closed surface.

(2) The mean value of the fourth power of the length of a chord of a closed surface.

(3) The mean value of the volume of a tetrahedron whose vertices lie inside a given closed surface.

The book will prove of value to the reader who is acquainted with the work of Poincaré, Borel, and Pólya.

Théorie Nouvelle de la Probabilité des Causes. By STANISLAS MILLOT. Pp. 35. Fr. 5. 1925. (Gauthier-Villars.)

The element of novelty in this new theory of the probability of causes is very slight. The author introduces the idea of a zone of probability in the following way. When an event occurs a times in m trials, Gauss' integral gives a measure of the degree of certainty that the probability, y, of the occurrence of the event satisfies the inequality $\left|\frac{a}{m}-y\right| \leq x\left\{\frac{2y(1-y)}{m}\right\}^{\frac{1}{2}}$, where x is the relative error. Taking the sign of equality and calling y the probability *à posteriori*, the author calls the region lying between the two branches of the above curve the zone of probability. He develops this idea and applies his work to certain examples discussed by Laplace. In these examples m is large, and it is sufficient to adopt the usual method of considering the tangents at the branch-point. As the few formulae developed by M. Millot are obtainable by this method it is not surprising to find that his numerical results are in close agreement with those of Laplace.

Calcul des Probabilités. By PAUL LÉVY. Pp. 350. Fr. 40. 1925. (Gauthier-Villars.)

This book by M. Lévy on the Theory of Probability is written in order to emphasise a point of view which seems to have been ignored by other writers on the subject. Its special feature is the prominent place given to the use of the characteristic function which Cauchy was the first to introduce. M. Lévy attempts to justify the fundamental principles of the theory of errors by giving a suitable precision to the intuitive idea of accidental error, and by deducing in a rigorous manner that the accidental error obeys the law of Gauss. Several writers think that this result does not justify the mathematical apparatus necessary to establish it, and the author attempts to combat this idea by showing that the mathematical methods required are not so formidable as is generally supposed.

The book is divided into two parts, the first of which is elementary and is devoted to setting forth the principles of the theory of probability. Here the author has been careful to state what is definition and what is mathematical reasoning. He has therefore emphasised the part which is subjective in the idea of probability, and he discusses the transition from the subjective to the objective.

The second part of the book gives a systematic account of the use of the characteristic function. The conditions, which are necessary in order that a function may be a characteristic function, are deduced, and the properties of a given law of probability are obtained from its corresponding characteristic function. The general results obtained are applied to the laws due to Gauss and Cauchy. It is doubtful whether this portion of the book will be appreciated by readers who have not acquired a considerable knowledge of analytical processes. As an application of the theory of errors M. Lévy has devoted a chapter to the Kinetic Theory of gases. Here he gives prominence to the idea of reversibility, from which he deduces Maxwell's law.

On the whole M. Lévy's work will be considered by many to be rather prolix. Readers who find the first part new and exciting are likely to be overwhelmed by the analysis in the second part, and those who can follow the second part with ease will find the first part rather dull. But the book contains a fund of valuable information, and the author has certainly succeeded in showing that the use of the characteristic function has a synthetic value which deserves a prominent place in the development of the subject.

J. MARSHALL.

Graphic Methods in Education. By J. H. WILLIAMS. Pp. xvii+319. 7s. 6d. 1926. (Harrap & Co.)

This book might well have been entitled "Graphic Methods in Advertising," for it is not a treatise concerning the use of graphic methods in school, but a considered account of the best methods of "displaying" information and statistics relating to educational administration. It is written for school principals, administrators, etc., and deals with the use of charts. Various forms of chart display are discussed and illustrated, and, in an indirect way, this might be useful to a teacher in helping him to appreciate the use of the blackboard in teaching. Some of the illustrations, however, are open to mathematical criticism, *e.g.* most of those in which magnitudes are represented by areas; it is much easier to compare lengths than areas, and columns (of equal breadth) are preferable to squares, triangles, etc., or to clusters of dots in which magnitude is indicated by density of aggregation.

Stories about Numberland. By D. PONTON. Pp. viii+128. 2s. 6d. 1925. (J. M. Dent.)

A series of delightfully fascinating stories dealing with early number work, decimals, weights and measures, money and simple geometry. There is both ingenuity and humour in the book, and it should make an appeal to the hardened mathematical teacher as well as to pupils of all ages. It should be in every school library.

R. S. W.

Model of an Abacus. By R. S. Williamson. 30s. 1926. (R. Platt, Ltd., Wigan.)

A Roman abacus in the British Museum has been Mr. Williamson's happy choice. His adaptation of it affords opportunities of explaining to young children the elements of numeration, and will teach them incidentally that the Roman system of integers was, rather cryptically, based on a decimal system. It also suggests that the Roman system of integers was not altogether illogical. Moreover, it illustrates vulgar fractions and the duodecimal table which persists from Roman times in our own measures.

The instrument should prove a useful adjunct to a mathematical museum. On occasions it will prove a stimulant to a class, and a pupil of ingenious mind may be left to play with it. He will probably find out some curious properties for himself, and that without causing any material damage to it.

A. J. Pressland.

SUMMER MEETING OF THE YORKSHIRE BRANCH.

The Summer Meeting of the Yorkshire Branch of the Mathematical Association was held at the Bradford Grammar School on Saturday, June 26th, 1926. A paper was read on "Cubic indeterminate equations," by Professor L. J. Mordell, of Manchester University. Mr. C. Cooper, of Skipton Grammar School, read a paper on "The introduction of algebra into the school course." It was decided that a discussion be held on Mr. Cooper's paper at the October meeting. Dr. W. E. H. Berwick, of Leeds University, was congratulated on his appointment to the Chair of Mathematics at the University College, Bangor, and Mr. J. H. Everett, on his appointment as head of the Leeds Central Technical School. The chairman, Professor S. Brodetsky, of Leeds University, gave a report of his negotiations concerning the celebration in March 1927 of the bicentenary of the death of Sir Isaac Newton. He mentioned that Sir J. J. Thomson, Master of Trinity College, Cambridge, the Astronomer Royal, the Bishop of Birmingham, Professor H. H. Turner, of Oxford, and Dr. J. H. Jeans, Secretary of the Royal Society, had consented to take part in the celebrations.

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389. Pray let me entreat you not to trouble your head about being a Senior Wrangler or any such stuff ; who knows or cares whether a man in any profession was a Wrangler when at Cambridge.—Sir William Pepys, Master in Chancery (to his eldest son William), 1798.

390. Sir W. Pepys, Master in Chancery, to his son William, 1798 :
I perfectly agree with you, that of all Scholars, None appear to be so little calculated for the Business of Life as abstruse Mathematicians. If you contract the Habit of reasoning more closely from your Mathematics, You have extracted all the Good from them which They can ever produce to You. But don't drop this Sentiment of mine out of your Pocket, for It would be burned by the Hands of the Cambridge Common Hangman.

391. Mathematics are pursued with a zest almost approaching passion by many artizans of our manufacturing towns. The booksellers will tell you of a constant demand for even antiquated treatises. The weaver still works with fluxions, in ignorance of the more convenient calculus ; and we may fear that many a brain while mechanically watching the whirling lathe . . . is wasting its strength in seeking to square the circle or discover the perpetual motion.—*Fraser's Magazine*, July 1854, p. 7.

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From Mr. **W. J. Greenstreet**:

P. W. Bridgman Thermodynamic Formulas - - - - - - 1925

D. R. Curtiss Analytic Functions of a Complex Variable Carus 2 1926

J. H. Jeans Atomicity and Quanta - - - - - - - 1926

From Rev. **H. Poole**, pamphlets by J. C. Adams, G. T. McCaw, A. A. Robb, R. Sheepshanks, and T. P. Thompson, a text-book by Lane, and the following books:

C. L. Dodgson New Theory of Parallels (4) - - - - - 1895

This edition reproduces the third, 1890, and differs substantially from the second, which follows the first, 1888, already in the Library.

W. R. Martin Navigation and Nautical Astronomy (3) - - - 1899

J. Perry Practical Mathematics ( ) - - - - - - 1910

Summary of six lectures to working men, much enlarged from the first edition, 1899.

S. B. Von Donop Notes on Mechanism as applied to Artillery (2: F. H. Crampton) 1902

From Mr. **Frank Robbins**:

W. H. Smyth Speculum Hartwellianum - - - - - - 1860

From Miss **M. T. Taylor**:

D. W. Scott Terra Firma: The Earth not a Planet - - - 1901

The following have been bought:

N. H. Abel Eine Klasse auflösbarer Gleichungen 1829: Ostwald III. 1900

W. K. Clifford Elements of Dynamics; I: Kinematic iv (R. Tucker) 1887

The earlier volume *Kinematic i-iii* was already in the Library. The subtitle of the second volume is illchosen, for the three divisions of the first volume are those into which Clifford divides the subject of Kinematic in his opening section.

T. H. Havelock Propagation of Disturbances in Dispersive Media Cambridge 17 1914

P. E. B. Jourdain The Principle of Least Action - - - - 1913

Reprint in one volume of three papers contributed to *The Monist*.

C.I.E.M. Reports: Allemagne 41, 42, 58.

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(*An Association of Teachers and Students of Elementary Mathematics.*)

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Its purpose is to form a strong combination of all persons who are interested in promoting good methods of teaching mathematics. The Association has already been largely successful in this direction. It has become a recognised authority in its own department, and is continuing to exert an important influence on methods of examination.

The Annual Meeting of the Association is held in January. Other Meetings are held when desired. At these Meetings papers on elementary mathematics are read and discussed.

Branches of the Association have been formed in London, Bangor, Yorkshire, Bristol, Manchester, Cardiff, the Midlands (Birmingham), New South Wales (Sydney), Queensland (Brisbane), and Victoria (Melbourne). Further information concerning these branches can be obtained from the Honorary Secretaries of the Association.

"The Mathematical Gazette" (published by Messrs. G. BELL & SONS, LTD.) is the organ of the Association. It is issued at least six times a year. The price per copy (to non-members) is usually 2s. 6d. each. The *Gazette* contains—

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